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ON THE CONTINGENCY OF NATURAL LAW

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ON THE CONTINGENCY OF NATURAL LAW

WHEN we are faced, as we always have been, with the problem of deciding whether we are to look upon nature as describable in terms of mechanical law, or to regard it as a system in which a present event is in some sense independent of past and future events, our decision must ultimately be based upon the sense in which we believe that we have knowledge of that nature. This commonplace, which serves to introduce our subject, has been emphasized and made explicit by many writers, among them Paul Painlevé, who (in *Les Axiomes de la Mécanique,* Paris, 1922), remarks that, "....to be really accurate, profound and philosophical, a critique of the principles of mechanics should be preceded by a general critique of the foundations of knowledge."

But we are not concerned now with the questions which, in their answering, made the 18th century empiricists differ from the 17th century rationalists. One must agree with Kant,—"There can be no doubt that all our knowledge begins with experience." We have to do rather with questions which must trouble all those who have come after Kant, if they have thought over the progress of the dialectic which marks the advance of modern philosophy.

Our decision must ultimately be based, then, on what we think to be the relation between nature considered in itself, and our knowledge of it, and this in turn must de-

pend upon what sort of knowledge we believe ourselves to have of nature. These questions, therefore, will earn our first consideration.

We have spoken of nature considered in itself, and an issue might be raised as to whether we have the right to do so. For if we follow the dialectic of history but a little way beyond Kant, we find, first with Salomon Maimon, later with Fichte, this nature to become absorbed into the self of the knower, and to lose any self of its own. We must, then, retrace our steps to see the motive for this change.

Kant, insistent upon the necessity of things-in-themselves, attempts to find some way of giving them meaning. But the very nature of his system stands in his way, and he leaves them in an impossible situation. The only escape, for those who wished to adhere to some of the fundamentals of his system, seemed to lie in getting rid of these things-in-themselves, and the only way to accomplish this seemed to be to bring them in among the 'things-in-ourselves'. This means of escape is, after all, a continuation of Kant's own endeavor, an attempt to find a way of defining the 'given', that *of which* we have knowledge. And since this is the essential problem,—one of definition,—it matters little whether or not we speak of a nature-in-itself, provided that we can define satisfactorily that to which we refer as 'nature',—that *of which* we believe ourselves to have knowledge. So this definition becomes a part of our problem.

When our attitude toward these epistemological matters has been made clear, there remain further questions upon whose answering our decision will depend. We must inquire next as to what sort of thing is a mechanical law, and as to how it differs from the statistical sort which

will provide our only means of describing a world in which events have no necessary connection. If there is anything peculiar to either sort which, when taken in connection with the kind of data which we have concerning events, makes that sort of law the only one eligible for their description, then our decision is immediately made.

If on the other hand it turns out that neither has such a peculiarity, that, in other words, either is possible, then the question becomes one of choice, and our decision must rest upon the reasons which make one more desirable than the other.

It is the last case which we wish to establish. In so doing, mention may well be made of some recent developments which seem to oppose this conclusion, and to set up the statistical sort of law as inescapable. That we may give some evidence of favoring (as a matter of choice) the other sort, is, perhaps, inevitable, since without some prejudice of the kind one would never have set out upon this endeavor.

I

The first question to be approached, then, is that which asks what sort of knowledge it is which we call our knowledge of nature. This knowledge involves perceptions, and we come to it through the senses. Are we then to have to do with perceptions in general, with the sensations of everyday life? Shall we analyze the experiences which we have when a tree comes within range of our vision, or our toe encounters a stone? These are indeed experiences, and moreover, they teach us something of nature. But while they serve the purposes of practical existence, and give us a knowledge not to be slighted, such perceptions are notoriously careless and casual. Certainly they are often accurate enough that we find each other to agree

as to what it is that we have seen or felt, but this may itself be a failing. Such facile agreement leaves something open to doubt. If experience never went further than this, there would be no problems like this to discuss.

The experiences we have need to consider are far from careless and casual; they are all that is refined and purposeful. Refined to such an extent that agreement must be carefully defined, in order that every 'experiencer' may not differ from every other; purposeful to the extent that one may well spend a great part of a lifetime in repeating one tiny experience, in order to feel sure of what he has learned. For such experience we must go to the expert, to the one who makes it his job. In other words, to the experimental physicist, who calls his experiences experiments, and considers accuracy his god.

If we go to this one at his work, and watch how he proceeds with his task, and consider the standards he sets himself, and what he makes of his results, we shall learn what is meant by having knowledge of nature, when this knowledge is the best to be had. And from what we have learned we shall be able, as we have seen that we eventually must, to define what is meant by nature, when nature is that *of which* experience is had.

In discussing what would be learned from such a tour of inspection, we are following almost directly what has been done in E. A. Singer's *Experience and Reflection*,[1] Part II of which, entitled "The Mechanical Ideal," has to do with the relation of experience and reality, so far as this last concerns what is commonly referred to as the physical world. We can make no claim here to originality, but must rather be thankful that we have so much of the way made clear for us. We turn our attention, then, to the experimenter at work.

[1]Not yet published.

From a consideration of the actual procedure of the experimenter in his investigation of nature, it is seen that certain features are characteristic of his results. In the first place, these results are not based on a single observation, but on a set of observations of the same objects under the same conditions. And since an experiment is not considered satisfactory in which each of a set of observations of the same quantity yields the same result, it follows that there must be some method of dealing with such a set. This, of course, is a question of probability, and since the science of probability has never been reduced to a foundation in an acceptable set of categorical postulates, there are different answers to the question. As, however, these answers vary in external form rather than in essential nature, the simplest may be used for illustrative purposes.

Suppose a set of observations giving different values for the same quantity. This set may be reasonably represented by the mean, but this does not tell enough of the story. The fact that the observations themselves differed more or less from this mean may be represented by determining the mean of their variation from the mean, and it may then be asserted that the experiment gives the value of the quantity measured as so much, the mean, plus or minus so much, this mean variation. Thus the experiment does not yield an exact representation of reality, but merely sets limits within which the real fact is supposed to lie.

But this is not all. An inquiring mind is not to be satisfied with a single result of this sort, and experimental determination of the same quantity will be made time and again. And these determinations will differ, both in the mean and in the mean variation, which may be said to stand, in this simple case, for the variable error of the ex-

periment. On what grounds, then, is one to be accepted as more accurate than another?

This is a question which might receive many answers, but which always calls forth the same one. That experiment is said to give a closer approximation to the real fact under consideration which offers as a result a mean value with a lesser mean variation, or in more general terms, a value with a lesser variable error.

What we have just called variable error is the result of the necessary inaccuracies in making readings and in controlling the conditions of observation. Other things being left out of consideration, this sort of error is always present because a given set of readings is confined to a finite number of decimal places, and the possibility remains that further readings to a next decimal will vary among themselves. As has been pointed out, this sort of error is brought under control by a reasonable multiplication of readings. The question of what constitutes a 'reasonable' number in this case may be partially answered by considering another sort of error which must be supposed to be always present.

This second sort of error we may introduce through a statement of Poincaré. "We have two series of observations; in applying the rule of least squares, we find that the probable error of the first series is half that of the second. The second series may however be better than the first, because the first may be affected by an enormous systematic error. All that we can say is that the first is probably better than the second, since its accidental error is less, and since we have no reason for asserting that the systematic error is greater for one series than for the other, our ignorance on this subject being absolute."[2]

A simple illustration of the presence of such system-

[2] H. Poincaré, *La Science et l'Hypothèse*, Paris, 1927, p. 242.

atic error in observation would be the case of a simple measurement of length with an inaccurate measuring rod, or the presence of an unsuspected magnetic field in an electrical experiment. Such sources of error may be partially eliminated by refinements in technique, by introducing conditions of symmetry, and by other expedients, but are no more to be supposed entirely escaped than are the sources of variable error. Of first importance in dealing with systematic error is to become aware in some way of the extent of its presence. One way in which this might be done suggests a method of defining a lower limit to a 'reasonable number' of readings.

Borrowing an illustration from Professor Singer, we shall consider an experiment performed with a pendulum. Systematic error creeps into this experiment through elongation of the suspending thread due to its being subject to a continual tension over a period of time. Suspecting the presence of this source of error, one might check it by considering the mean variation of the whole set of readings for the pendulum, in comparison with the mean variation among members of a part of that set. That the mean variation of the whole set turned out to be greater than that for the sub-set, would be evidence of the presence of such a source of error. The general question as to whether such evidence might always serve to establish the presence of more or less systematic error, we shall not consider here. What is to be pointed out is that for such a procedure to be possible, there must be a certain minimum number of readings. The least number of readings which permits of finding a mean and a mean variation is n-2. The least number for which there is a subset containing two members is n-3, hence this must be the minimum number of readings.

Because of the presence of systematic error, the statement in a preceding paragraph of the relation of experimental result to fact is not entirely adequate. But if one takes into consideration the possibility of detecting this sort of error through a consideration of the behaviour of variable errors, and of 'weighting' experiments according to a suspicion of its presence, then it seems to have expressed the spirit of the matter. That experiment is said to give a closer approximation to the real fact under consideration which offers as a result a value with a lesser probable error.

Such are the essential features of the experimenter's procedure. From them Professor Singer draws certain simple and straightforward conclusions. First, that there is some fact (which we may call real or objective) seems to follow from the use of the correlative term, as Kant himself has argued.[3] "Observation" by itself is a meaningless term. "Observation-of-fact" has meaning. So if there is to be any sense to observation, there must be a meaning to fact.

That there is a meaning to fact quite consistent with the procedure by which one seeks to attain it is easily seen, and fact, that is, physical fact, may be defined as "that which the mean lies nearer to, the nearer its variation lies to zero".[4] Here, it is understood, approach to reality is not defined in terms of what reality is conceived to be, but the real fact is defined in terms of an actual and mean-

[3] *K.d. r.V. P. 251-A ed. ".....,und es folgt auch natürlicherweise aus dem Begriffe einer Erscheinung überhaupt; dass ihr etwas entsprechen müsse, was an sich nicht Erscheinung ist, weil Erscheinung nichts für sich selbst und ausser unserer Vorstellungsart sein kann, mithin, wo nicht ein beständiger Zirkel herauskommen soll, das Wort Erscheinung schon eine Beziehung auf etwas anzeigt, dessen unmittelbare Vorsellung zwar sinnlich ist, was aber an sich selbst, auch ohne diese Beschaffenheit unserer Sinnlichkeit, (worauf sich die Form unserer Anschauung gründet,) etwas, d.i. ein von der Sinnlichkeit unabhängiger Gegenstand sein muss."*

ingful approach. In this light the situation may be summed up as follows: "The fact of exact science is never given in experiment, it is always present to the experimenter as the ideal which makes his 'observations' possible."[4]

There is a certain matter which is needful of emphasis at this point. In ordinary usage, the term error refers to a deviation from some known fact. That this cannot be its meaning here is evident from the assumption with which we start, that the fact itself can not be known. The 'error' which we have been considering is then quite independent of any knowledge of the true fact under consideration; it derives it whole meaning from the observational readings themselves, or, in the case of systematic error, from the conditions of the experiment.

Another misunderstanding to be avoided is this,—that, results being arranged in the order of decreasing probable error, the fact is to be looked upon as the limit of a series, in the mathematical sense. No such series is given; there is no means of determining from preceding terms what a later term might be, so that such a series cannot be defined. All that is given is a set of results, with a means of establishing order in this set. With a new result to be considered, the question is whether or not it may take its place at the end which is taken to be nearer to the fact. The fact plays the rôle of a limit, not as a conception of a limit, but rather as a limiting conception.

At the basis of this point of view there are certain definite propositions, which we hesitate to refer to as playing the part of assumptions; they may be listed as follows: (1) Experimental error can never be escaped. (2) Experimental error can always be reduced. (3) The unattainable but always approachable limit to the reduction of experimental error is zero.

[4] Singer, *op. cit.*

In this last proposition a distinction must perhaps be made. By limit is meant that to which the approaching quantity may come closer than any given amount. We mean by this to exclude the case where some quantity other than zero might appear as a barrier to the reduction of error, while, since this quantity would be unattainable and approachable, the quantity zero might also be said to have these characteristics. If such a quantity did appear, it would be referred to as the limit.

We might at this point ask a question, the answer to which seems to follow immediately from what has been said of the nature of the knowledge which the exact scientist has of the things with which he is concerned. The question,—"Can any experiment determine a threshold beyond which observational error cannot be reduced?" The answer,—"No."

If we should find an affirmative, instead of a negative answer to this question, our whole previous discussion would come to naught. For one of the presuppositions of this discussion was that "the unattainable but always approachable limit to the reduction of observational error is zero." It should be recalled that this presupposition is necessary in order that there may be a definite meaning to fact. And without this definite meaning, there is no sense in speaking of observation, or of knowledge.

But such arguments are, after all, not entirely satisfying. Though one of necessity seems to follow directly from another, and the whole only from the requirement that there be a meaning to the problem of knowledge in general, the method of approach seems indirect. Not that it is any the less binding, but that it involves an ultimate assumption which might conceivably be disputed, is the ground for supplementing it with argument which involves only the elements of the question proposed.

Such argument might take the following form. *No* experiment *can* set a limit to the reduction of error, because the result of that experiment would itself involve some error. And this same error leaves room for a freedom to choose an interpretation of that result which denies a limit to such reduction.

That the result of the experiment would involve some error is, we think, beyond contradiction. It must be granted on the basis of the physicist's actual procedure, when he is seeking accuracy in his measurements. The alternative, an experimental result with no variable error, would imply that, each reading having been carried out to an infinity of decimal places, all had been found to agree. For agreement to a given decimal place is but the forerunner of disagreement in the next, or the next, or the next, and an experiment has not been carried out until this disagreement is found, or until the infinite 'all' of the decimal places has been gone through. Unless this latter is to be admitted possible, then, any experimental result must carry with it some variable error. That the presence of this error leaves room for a freedom to choose seems equally clear. There is not only a freedom to choose, but also a necessity of choosing. That among the possible choices there is one at least which will permit us to deny a limit to the reduction of error is perhaps a more subtle point to decide.

The number of these possible choices may be said to be infinite. If, then, our decision as to the possibility of reduction rests solely upon our interpretation of this particular result, then assuredly we could find the one we want. If, on the other hand, as must be the case, it depends on some theoretical considerations, which are to be made definite by the conjunction of this result with

other results, we still seem to have sufficient freedom to find the one we want. For these other results, likewise involving error, admit each an infinity of interpretations, and the only problem is to find one interpretation for each result which will conform to the laws of a theory which sets no limit to the reduction of error.

That previous interpretations may have to be rejected is no stumbling block. They have themselves been chosen for some reason,—to satisfy some methodological ideal. One might, it is true, wish to weigh the urgency of our demand for unlimited reduction of error against the desirability of this ideal, but this is a problem which need not concern us here. The question is,—Can certain interpretations be found? The answer must be that they can. Hence the answer to the original question: No experiment can set a limit to the reduction of observational error.

Later on we are going to discuss experiments which, it is claimed, do set such limits. That these claims are based upon a misunderstanding of the problem, and upon false motives, it will be our task there to prove.

II

It now becomes our problem to consider what differences there are between mechanical and statistical descriptions, and to get at the defining characteristics of each. We may begin by taking each in its turn, considering its limitations and its possibilities, and end by setting up one against the other, in order to bring out the particulars in which they differ.

The essence of mechanical description is suggested by the following passage,—so often referred to in discussing these matters,—from the *Essai philosophique sur les probabilités* of Laplace. "An intelligence which, at a given instant, knew all the forces by which nature is moved and

the respective situations of the entities of which it is composed, would, if besides it were great enough to submit these data to analysis, embrace in a single formula the movements of the largest bodies in the universe and those of the lightest atom. For this intelligence nothing would be uncertain, and the future, as well as the past, would be present to its eyes." This all depends, of course, upon an 'if'; it is merely a hypothetical proposition. That such is the case need not disturb us greatly; it merely indicates that a complete mechanical description has something about it of the 'ideal'.

At present our interest is drawn to those aspects of this view of nature which cause us to refer to it as a mechanical one. The essential feature would seem to be that of necessity, which makes the past and future exactly and completely determined by the present. Another feature is that of universality. It involves the lightest atoms, as well as the largest bodies. And still another is that it is the situations of these beings, and their movements, which are involved. All of which leads to the usual paraphrasing of Laplace's view of nature in some such terms as these,—"The mass, position and velocity of every particle in the universe being known at a given instant, and the laws governing their behaviour, then these properties may be calculated for every particle for any other instant, past or future." And this is but a way of defining what is meant by mechanical *law*.

We might base our discussion on this rather loose way of stating the essence of mechanism, but that more definite, yet more general, statements of it are to be had. We may object to the particular conditions of mass, position and velocity, which are to be known, and we may wish to avoid the looseness of the word 'particle'. Hence we shall

choose a way which generalizes in the one case, particularizes in the other, and is, on the whole, more definite in its statement. Best for our purposes is that of Professor Singer, who, in an essay on "The Philosophy of Experiment," sets up a definition of a "mechanical image of nature." We shall put down this definition, and attempt to gather from the image those essentials of mechanical law which may or may not make that sort of law the only sort eligible for the description of natural events.

"A mechanical image of a natural system is constituted of a manifold of points, individuated by space-time coordinates, classified in terms of a finite number of dimensions, and subject to a law such that conditions at all points being given for a value t_1 of an independent variable t they are determined for any second value t_2. The t for which these requirements hold true is the *time-dimension* of the space-time coordinates individuating the points of the system."[5]

If this is to be a mechanical image of nature, what of the sort of law which it involves? Or more to the point, what of the sort of law which one has to consider a possible one, if he is seeking to establish such an image? For our problem must not get too far away from that of the scientist, who is trying to find some way of describing events as he is, or may become, aware of them. If he is convinced, first that such an image is possible of approach, secondly that it is desirable, then the laws which he employs in his describing must be such as will fit into such an image. It is into the nature of these laws which he must use that we are inquiring.

The first requirement imposed on these laws is that they are to deal with *points*. It is the points in a mani-

5E. A. Singer, Jr., "The Philosophy of Experiment," *Symposium*, Vol. I, No. II, April, 1930, p. 161.

fold which are to be subject to law. There are but two matters to dwell upon in connection with this requirement. The first is simply the matter of emphasizing the fact that it is the individual which is the prime element in mechanical description,—the individual in its most elementary form. Laplace, in the statement of his view, was concerned with individuals, and he was probably thinking of points. When he speaks of the largest body and the smallest atom he means to refer to individuals, and he means to consider the various attributes of these 'bodies' as associated with points. Now we are speaking explicitly of points, and meaning all points and the attributes which may be associated with each.

The second matter arises naturally out of the first, with the problem of recognizing an elementary individual as such. Essentially this is a dual problem. On the one hand, there is the question of *individuation,* of recognizing the individual as distinct from any other individual; on the other the question of *identification,* of recognizing it as the same individual at different times in its career. In both cases, the individuals which are the subjects of mechanical law are recognized by determining where they are at a given time. In the first case, a point p_1 is to be distinguished from every other point by being in the position P_1 at a given time. If any other point be supposed to occupy that same position at the same time, then it cannot be another, but must be the same point. Again, a point p_1 at a moment $t_1$is to be recognized as the same point as the point p_2 at the moment t_2 by tracing its continuous path through the intervening continuum of point-positions from the position P_1 at the time t_1 to the position P_2 at the time t_2. The first of these conditions for recognition involves merely the determination of position, to

which we shall return when we come to the question of the determination of dimensions in general. The second condition, however, that which has to do with identity, imposes a requirement on our laws. For the problem of tracing a continuous path is equivalent to that of setting up an equation describing the change of position with time, and the equation bears a direct relation to law, since some law must account for the behaviour of the point in this way. The requirement placed upon the path is that it be continuous; that placed upon the equation and the law, that they involve no discontinuities, that the change of position be expressible in terms of a continuous function. This element of continuity, then, must be characteristic of any natural law which we are going to call mechanical.

The next important requirement in the definition of a mechanical image from which we are trying to draw our notion of mechanical law is that the points which are subject to such law are to be "classified in terms of a finite number of dimensions." The question here is not one of what these dimensions are going to be. This is a question which could only be answered if the mechanical image of some observed system of nature were taken to be established. History shows that in the course of pursuit of that image, the dimensions employed in attempts at mechanical description have varied at different stages of development. The motives which prompt this change in the way of classification, while they might give some indication of what the ultimate dimensions might have to be, still do not affect our immediate problem.

We find this problem again when we remember that we are most vitally interested in the scientist who is seeking to describe. For him these dimensions become material with which he has to work. As an experimenter it is his

business to measure them, and to give them particular values in particular instances. The result of his measuring becomes that which he is given, that knowledge which he has of events, present and past, which he is trying to relate by some sort of law. The problem of dimensions associated with points which are subject to law becomes for him a problem of observation; observation which is carried out for the purpose of finding a possible classification for particular points under consideration. Among these dimensions which must be measured are to be included those which serve to individuate,—the space-time coordinates of the points in question.

This is a place to recall what has been emphasized on an earlier page concerning what is 'given' to the experimenter. It was pointed out there that his 'observation-of-fact' always yields a result with a probable error. This result is capable of an infinity of interpretations, and the problem in establishing a law is that of choosing one definite interpretation for each fact observed. There is no question of treating the whole range of values included by the probable error as a group which is to be brought under some law. Rather, one single value for each dimension is chosen from within the range, and taken to be *the* value because it can be brought under an acceptable law. The problem is analogous to that of drawing a plane curve which shall pass through a certain number of small, finite one-dimensional regions distributed over a surface. One considers the possible curves which fulfill this condition, and chooses that which is most desirable because of its fulfilling certain other conditions. The regions correspond to what is 'given' to the experimenter, the points in these regions which become part of the curve are the values which he chooses as his interpretation of that 'given'.

The next feature of mechanical law to be considered is definitely indicated in the definition of a 'mechanical image'. The law is to be "such that conditions at all points being given for a value t_1 of an independent variable t they are determined for any second value t_2." If t is the variable representing time, then this condition amounts to the requirement that a law, established on the basis of past events, should be able to predict for events in the future, and to describe other events in the past. Since to predict and to recall are essentially the same, depending on whether the time is to be taken as positive or negative with respect to the present, we shall refer to this requirement as that of predictability.

The important question in this connection is the sense in which mechanical law is said to predict. The answer is quite evident from our image. Just as this sort of law is established for points, and for definite dimensions associated with these points, so is it supposed to predict. In the completed image, the law is such that it applies to all points. In the practical case of a law established for conditions at certain points, this law is supposed to predict conditions at certain other points. If the law is analogous to the curve suggested a moment ago, then it predicts in the sense that that curve, its form having been determined, fixes certain points as belonging upon it, if it should be continued into their neighborhood.

A prediction attains to meaning by being subject to verification, and we must ask in what sense the predictions of mechanical law can be verified. The answering of this question takes us back again to the problem of observation and measurement. Certain observations must be made in order to verify a prediction. These observations will yield a result which, involving error, is capable of an infinity

of interpretations. If now the definite conditions predicted for a particular point at a given time are to be found among the possible interpretations of the results of observations of the conditions at that point at that time, then these observations are considered to verify the prediction. This is simply the meaning of the much used phrase,—'verified within the limits of probable error'.

It is important to note the relation of this sort of verification to the validity of the law. There is a vast difference in this respect between the consequences of an affirmative result which verifies a particular prediction, and a negative result which fails to verify it within the limits of probable error. The first is confirmation of the law; it is evidence in its favor, but it does not establish its validity with certainty. There always remains the possibility that the law will fail in some other prediction. This affirmative result is negative in its effect. It neither serves to establish the validity of the law, nor does it bring about any change in the law at all.

The negative result, on the other hand, failing to verify the prediction, immediately forces the rejection of the law, just as the behaviour of the planet Mercury has forced the rejection of the Newtonian law of gravitation. And the fact that the general theory of relativity accounts for the behaviour of Mercury as well as for other gravitational phenomena does not prove with certainty the validity of that theory.

Before leaving the subject of mechanical description and the nature of mechanical law, there is still one point to be considered. A considerable misunderstanding may arise from confusing the problem of induction with that of what may be called statistical description. Induction may be defined in a general way as the form of inference

in which one passes from the individual to the universal. Its essential nature as a method in science is portrayed in Newton's 'Four Rules for Reasoning'. And one is sufficiently convinced of the compatibility of this method with the attempt at mechanical description when one considers that Newton, to illustrate these rules, makes use of the arguments by which he arrives at his law of universal gravitation, than which none could be more mechanical, and less statistical.

Because a particular mechanical law is only one of a number of possible laws, it may be considered as more or less probable than others of this number. But the probability involved is not inherent in the law, in any sense that would make the law less mechanical. Rather, the probability refers to the degree in which this law may be taken to be an approximation to truth. This is certainly what Poincaré means when he makes the following statement. "For all these reasons, no particular law will ever be anything but approximate and probable. Scientists have never failed to appreciate this fact; but they do believe, whether rightly or wrongly, that every law may be replaced by another more closely approximate and more probable, that this new law will be itself only provisory, and that the same movement may continue indefinitely, so that science as it progresses will possess laws which are more and more probable, and that the approximation will end by differing as little as one please from exactness, and the probability as little as one please from certainty."[6]

This probability may be seen to rest on many factors, depending upon who it is that is doing the seeing, but it has no ground in the form of the law. From the point of view of anyone, confirmation of a law by the experimental verification of a prediction increases the probabil-

[6]H. Poincaré, *La Valeur de la Science,* Paris, 1925, p. 251.

ity of that law. Another sense in which one law may be more probable than another is to be found in considering the sort of data upon which the law is based. Not because of what there may be of a statistical nature about the formulation of these data, but because of the relative accuracy of the data in the case of two alternative laws, may one be considered more probable than another.

There are plenty of other reasons for ascribing more or less probability to a law. For Galileo, simplicity in the form of a law increases its probability, because he feels that it is "the habit and custom of nature herself—to employ only those means which are most common, simple and easy."[7] For Newton a law is more probable because it can account for phenomena whose description might otherwise require more than one law. For Ernst Mach it is a question of economy, of the saving of thought and experiences, which in turn involves simplicity. Poincaré recognizes simplicity and unity as criteria of probability when, in regard to two opposing tendencies in the history of the development of physics, one towards unity and simplicity, the other towards variety and complication, he says,—"Of these two inverse tendencies which seem to triumph by turns, which will win out? If it is the first, science is possible...."[8] Pierre Duhem considers those laws more probable which lend themselves best to classification. "Thus physical theory never gives us the explanation of experimental laws; it never reveals to us the realities which are hidden behind sensible appearances; but the more it is perfected, the more we feel that the logical order in which it arranges the experimental laws is the reflection of an ontological order; the more we suspect that the relations which it establishes between the

[7]Galileo, *Two New Sciences,* Trans. by Crew and De Salvio, 1914, pp. 160-161.

[8]H. Poincaré, *La Science et l'Hypothèse,* p. 203.

data of observation correspond to the relations between things, and divine that it is tending toward a natural classification."[9] And Professor Singer has shown that with all the atomists, the motive for preferring one particular description of nature to another is the reduction of the number of dimensions in terms of which nature is to be described. Hence for them the reduction of dimensions is the mark of a more probable law.

The very multiplicity of these grounds for according a higher degree of probability to one law than to another is sufficient evidence that this probability is not something characteristic of the law, and seems to show that it refers to something outside of the law. A mechanical law may be more or less probable, without being a law of probability.

This confusion between two uses of the term probable may be somewhat removed by introducing qualifying adjectives which are suggested by Cournot. That sort of probability which involves the ratio of the cases favorable to those possible, and which may be expressed by the fraction k/n, is called *mathematical.* It is characterized by two limiting cases, one called *certainty* and the other *impossibility.* From a logical standpoint, certainty would be attained in a case where k=n. Impossibility would be indicated if k=o. Moreover, there would be no probability expressed if k=1, unless induction from a single instance is to be recognized as valid. Hence the conditions that the fraction k/n express a probability is that $1<k<n$. Besides these absolute limits which have their ground in reason, Cournot suggests another sort as being more useful from an empirical point of view. He defines an event as *physically certain,* when the probability of the occurrence of this event differs from unity by a quantity less than any

[9] P. Duhem, *La Théorie Physique,* 2nd Ed., 1914, p. 35.

given quantity. Likewise, the event is *physically impossible* if it has only one chance out of an infinity of chances of taking place.[10]

In the light of these definitions, it would appear to be meaningless to speak of the probability of a particular experimental law. There being an infinity of possible laws interpreting the same or different data, and only one, if any, of these being capable of being *the* law, it seems to be *physically impossible* that any particular one should be If there is to be any meaning in speaking of one law as more probable than another, then it must be in some sense other than that of mathematical probability. To find some other sense is just what Cournot attempts in introducing his notion of *philosophical probability.*

The best understanding of this notion should follow upon a consideration of its author's own treatment. We shall try to bring together enough passages from his most important philosophical work to create this understanding.

"For example, certain physical theories are, in the present state of science, held to be more probable than others, because they seem to satisfy better a rational concatenation of observed facts, because they are simpler or because they bring out more remarkable analogies. But the force of these analogies, of these inductions, does not strike all minds in the same degree, even the most enlightened and the most impartial. The reason is struck with certain probabilities, which do not, however, suffice to fix a complete conviction. These probabilities change with the progress of science. A certain theory, rejected in the beginning and then struggled against for a long time, ends by obtaining unanimous acceptance. But some give in later than others, which shows that there is in

[10]See A. Cournot, *Essai sur les Fondements de nos Connaissances*, Paris, 1851, Ch. III and IV.

the elements of this probability something relative to the mind which recognizes it."[11]

"This subjective and variable probability, which sometimes excludes doubt and engenders a certitude *sui generis,* and which at other times appears only as a vacillating glimmer, is what we call *philosophical probability,* because it involves the exercise of that superior faculty by which we take account of the order and reason of things."[12]

"But, on the other hand, philosophical probability is essentially different from mathematical probability, in that it is not reducible to numbers: not because of the present imperfection of our knowledge in the science of numbers, but in itself and of its own nature. There is no opportunity to number the possible laws, by means of the continuous or the discontinuous variation of some numerical element, nor to arrange them in rank, as magnitudes, in respect to that property of form which constitutes their degree of simplicity, and which gives, in different degrees, unity, symmetry, elegance and beauty to the theoretical conception of phenomena."[13]

Enough has been quoted to make superfluous any effort to point out how the conception of philosophical probability finds a place in our discussion. Two more sentences will serve to clarify a particular point with regard to its use here. From the first of Cournot's philosophical works we may take the following statement:—"Philosophical probability rests, without doubt, on a general, and a generally true notion of what things must be;. . . ."[14] One would like to say that a notion which has to do with 'what things must be', since it would be impossible of verification by any experience, is a meaningless one. Hence we are glad to

11 A. Cournot, *Essai,* § 50.
12 *Ibid.,* § 51.
13 *Ibid.,* § 52.
14 *Ibid.,* § 52.

find, in his last work, a different statement of Cournot's views. "However, all these *probabilities,* which elsewhere we have called *philosophical* (not daring to say *rational probabilities*), are founded on a preference of the reason for that which seems to it to satisfy best the conditions of simplicity, generality and symmetry, which contribute to perfection of order in all things."[15] The earlier 'notion' was meaningless, because it seemed to involve a question of fact which is unanswerable. This later 'preference' is another sort of thing, involving no question of fact, but rather questions of motive, of ends, of methodology. Hence it escapes being meaningless, since it is only of questions of fact that one would demand that to have meaning, they be verifiable by experience.

The same treatment applies equally well to the two sorts of probability. Mathematical probability involves a question of fact, and gets its meaning only by being subject to verification. What we are calling philosophical probability, on the other hand, has not this test to meet. And fortunately so, if it is going to preserve its meaning. Hence one is asking two entirely different sorts of question when one speaks of a 'more or less probable' law, and of a law of probability. And this is all the more reason for not confusing them.

There remain to be pointed out the reasons why it seems that description of nature in terms of mechanical law may be always possible. First to be considered are those reasons which have their ground in the nature of the 'facts' which are to form the basis of the description.

It seems to follow, from the statement of the case in the opening chapter of our discussion, that, 'facts' being what they are, they can offer no obstacle to mechanical description. If what is given is a limited range, within

[15] A. Cournot, *Matérialisme, Vitalisme, Rationalisme,* Paris, 1923, p. 250.

which the fact is supposed to lie, and if, for purposes of description, any of the infinity of facts to be found there may be taken to be *the* one, then it would seem that the sort of law to which the fact is to conform is as much a matter of choice as the fact itself. As was suggested by an illustration, the choice of fact might even be determined by a previous choice of law.

These facts are, of course, the dimensions referred to in the definition of a mechanical image of nature. Among them are included those dimensions which serve to individuate, by fixing coordinates in space and time. A particular characteristic of mechanical law, which arises from the necessity of this sort of individuation, will serve to illustrate how the nature of what is given makes it impossible for that 'given' to exclude the possibility of mechanical description. It followed, from the necessity for space-time individuation, and from the nature of mechanical identification, that mechanical law must have the property of continuity.

That the requirement of continuity can always be satisfied one is forced to admit, from consideration of a fact which is familiar to all mathematicians, and which we shall state here on the authority of Henri Poincaré. And with its statement we shall give the reason for its force in this particular connection. "But the numbers which the physicist measures by experiment are never known by him except approximately; and besides, any function whatever always differs as little as we wish from a continuous function."[16] This obviously means to say that within the limits of probable error there is plenty of room for description in terms of either sort of function, continuous or discontinuous.

Not because it has any bearing on our problem of in-

[16] H. Poincaré, *La Valeur de la Science*, Paris, 1925, p. 154.

vestigating the possibility of mechanical description, but because of its general interest as establishing a motive for preferring that sort of description, we shall cite another statement from the same author. In speaking of a tendency to consider a simple form of law more probable than one more complex, he says,—"....What remains of this tendency is the belief in continuity, and we have just seen that, if this belief should disappear in its turn, experimental science would become impossible."[17] For the reasons for coming to this conclusion, we are content to refer to other parts of the same work.

Aside from the 'facts', or the dimensions which enter into mechanical law, there is the requirement that the law refer to points, or to elementary individuals. Can one always associate dimensions with points, so that mechanical description may be possible? The answer would seem to depend on an *if*. The nature of this *if* is apparent from the following illustration. Newton's law of gravitation finds a certain force of attraction to exist between two spheres, in accordance with the formula, $f = \frac{mm'}{r^2}$. The r in this case is the distance between the centers of the spheres, and the masses m and m' are considered as concentrated in these points. But Newton's law refers to all particles, which we may take to mean all points with which mass may be associated, so that the formula applies equally well to any parts of the two spheres. Thus from one point of view the attraction may be considered as existing between the spheres as a whole, from another as between them as the sum of their parts. Mathematically, it can be shown that these two points of view yield the same result, but only on the basis of an assumption. For this equivalence to hold, the mass in each sphere must be distributed symmet-

[17] H. Poincaré, *La Science et l'Hypothèse*, Paris, 1927, p. 239.

rically throughout the whole sphere. Either they must each be homogeneous, as regards distribution of mass, or the density at different points must vary in a perfectly definite way, so that, for instance, concentric shells would exhibit homogeneity.

The problem then of associating dimensions with points is the problem of defining the distribution of any property within a body. And this in turn depends on the introduction of postulates of homogeneity or heterogeneity. Hence we would answer,—one can always associate dimensions with points, *if* one can postulate the desired homogeneity or heterogeneity of distribution. And such postulates are always possible, since they are only necessary to take the place of questions which experience can not answer.

Considered in this light, any objection to associating properties with bodies which are not themselves directly observable loses its force, since it becomes an objection to the practice on which all physical science is built, of referring properties to points. And it would seem that to offer the former objection would be to condone the latter.

If we grant, then, as it seems we must, that the properties which are subject to observation may always be referred to points, and that the determination of these properties by observation leaves open a possibility of choosing such as fit into mechanical description, there appears no reason why, in any stage of scientific development, the description of nature by mechanical law should be looked upon as impossible. The same conclusion is reached in Duhem's *L'Évolution de la Mécanique*. "If then the analytic method proposes simply to reduce the explanation of physical phenomena to a mechanics in which the notion of motion, mass, and force are taken to be the prime notions; or even if it proposes to give this explanation

without appealing to the notion of force, there is no law of experimental origin which can be proved to be incon sistent with such an explanation." (Page 183).

III

Turning now to the problem of statistical description, there is again the task of discovering the characteristics of a type of law which may or may not make it the only sort elegible for a complete description of nature. In this case we shall not concern ourselves with the mathematical form of the law, but rather be content with pointing out certain features which make it inherently different from mechanical law. These differences will be sufficient to provide the grounds on which we may arrive at some decision with regard to our fundamental question.

The phrase statistical description is decidedly ambiguous, and it is part of our task to clarify its meaning. The first distinction might be made between description which is teleological in character, and that which is ateleological. In accordance with tradition, we take teleology to connote a theory of purposeful behaviour. The mere fact of one event following necessarily upon another, while it perhaps allows us to say that the first occurred in that order that the second might, does not convey the idea of purpose. Rather, one would say that the term purpose is characterized by the possibility of any given purpose not being fulfilled. Hence teleology is ruled out of mechanical description, although it is far from our purpose to insist that it is incompatible with a mechanical system.

Thus teleology must be dealt with only by those sciences which can find no definition in terms of mechanism for the classes of objects with which they deal. But this is not to say that statistical description is limited to objects which are to be defined only in terms of purpose, and hence

to be considered free, or undetermined, in their behaviour. Nor would one argue that because statistical description is possible for the behaviour of certain objects, they possess this freedom. There is nothing in the nature of the case to force an identification of problems of statistical description with problems of teleology.

In this connection there is another issue which must be brought to the fore. Statistical description, of course, bases itself on the mathematical theory of probability. And writers from the very beginning, with but few exceptions of which we are aware, have insisted that the laws of chance, as they are frequently called, are merely a confession of ignorance. The possibility of their existence is relative to the extent of our knowledge, or perhaps to the structure of our minds. This is not to say that they are less useful. One whose name may be taken to give authority finds the opposite to be the case. "You ask that I predict for you phenomena which are going to occur. If, by misfortune, I knew the laws of these phenomena, I could do this only by performing impossible calculations, and I would have to give up. But, since I have the good fortune not to know these laws, I will answer you immediately. And what is most extraordinary, my answer will be correct."[18] For this one, too, chance and its laws have their seat in ignorance. Among the exceptions mentioned above is of course Cournot, who, in opposition to the usual subjective theories, takes the realist view that chance is inherent in nature. This view we shall also find expressed by very recent writers.

It would seem that to choose now between these two views would be to beg our whole question. The ignorance of which we have been speaking is, as suggested by Poincaré, an ignorance of mechanical law. To assert that this

[18] H. Poincaré, *Science et Méthode*, Paris, 1927, p. 66.

ignorance is due to a peculiarity of nature itself, as we should if we agree with Cournot, would be to assume that mechanical description, in the last analysis, is impossible. The most reasonable course is to assume the two problems identical, and let our answer as to whether chance is or is not inherent in nature depend on our answer to the question as to whether or not only one sort of description is possible.

We might at this point insert a parenthetical remark to the effect that, if only statistical description is possible, the answer is affirmative; if only mechanical, it is negative; if both, neither, since the question becomes meaningless.

Returning now to the more important differences between mechanical and statistical law, we shall consider in particular three. One is concerned with the entities with which the law has to do, another with the nature of the law itself, and the third with the sense in which statistical law may be said to be true.

In the first case, the difference is quite marked. Whereas mechanical description is found to deal with the particular individual and its characteristics, statistical description, in so far as it deals with individuals at all, refers in its statements to the average behaviour of individuals considered as making up a group, or to the singular regarded as *any* individual, and to the probable characteristics of that (undetermined, and still to be individuated) individual. An example of the first case would be certain developments in thermodynamics with which everyone is familiar. The second case is illustrated by that step in the development of modern theories of atomic structure which takes the relative intensity of a given line in the spectrum of a particular element to measure the probability that any atom of that element be in a particular en-

ergy state. Or it might be illustrated by the mortality tables, which assign to a given individual a probability of attaining to a certain age.

Mechanical law we found to be confined to individuals which depended for their recognition upon their localization in space and time. Each individual must be distinguishable from every other, and can be brought under the law only as a separate and distinct entity. The objects of statistical law, on the other hand, may be subject to an entirely different means of individuation. Their sole distinguishing feature may be their membership in a certain group; they may be 'hydrogen atoms' or 'men', but not 'this hydrogen atom' or 'this man'. They are distinguished only from objects outside of their own group; not from those within. The law is established on the basis of an assumption that one member of the group is as likely to satisfy certain conditions as any other. All individuating characteristics are wiped away by bringing the individual into the group. Its definition as an individual is its definition as a member of the group. It might be suggested that the same may be said in the case of mechanical law. Newton's universal law, that 'all' particles attract each other in a certain way, refers to the members of a group, to points whose defining characteristic is that they have mass. But the derivation and the application of this law depends not only upon the recognition of certain points as members of the group, but also upon a consideration of the differences between such points. A particular point behaves as it does in conformity to such a law only because it is particular. Its conformity to statistical law, on the other hand, would depend not upon its being different from, but upon its being like, other such points.

This difference between the entities with which the dif-

ferent types of law have to do suggests that whether one is going to use one type or another in a particular case depends upon the point of view from which this case is to be considered. An example might be found in the kinetic theory of heat. From one point of view, the molecules in a gas may be considered as subject to mechanical law, acting under forces of attraction and repulsion. In this case the identity of each molecule is important, and the description of the behaviour of all of them will depend upon their being individuated. From another point of view, where one is interested in the behaviour of the group as the result of the individual behaviour of the molecules, one might, by making assumptions which are independent of the individuality of molecules, arrive at a satisfactory conclusion in terms of probabilities.

The second difference to be discussed has to do with the natures of the different sorts of law. By 'nature' we might mean mathematical form, except that we wish to confine ourselves to more general terms. Jean Nicod, in his work on *The Logical Problem of Induction,* makes a distinction between two types of inference. Considered as grounded in implication, which is "a connection between premisses and conclusion which asserts that the conclusion is true if the premisses are true,"[19] an inference is *certain.* Another connection, which indicates that if the premisses are true then the conclusion is probable to a certain degree, is the ground for *probable inference.* Of this type it is to be noted that "it does not conclude *in a probable manner,* but it reaches a p*robable conclusion* in as certain a manner as the other reaches its conclusion."[19] In both of these cases the premisses are taken to be true. The alternative is also to be considered. "We can even assert that starting from premisses which, taken

[19] J. Nicod, *Le Problème logique de l'Induction,* Paris, 1923, pp. 12, 13.

together have a probability p, a certain inference will confer on its conclusion the same probability p; and a *probable inference,* which would confer on its conclusion the probability q if its premisses were certain, will confer on its conclusion the probability pq."[19]

Since, in the case of mechanical law, we have insisted that the premisses, that is, the data, in so far as they enter into the law, are to be looked upon as certain, in the sense that they involve no probability, and since there is no probability attached to any prediction arising out of this law, then this type of law may be taken to be analogous to 'certain' inference from 'certain' premisses. It would seem that, if the analogy is to be carried further, any of the three remaining possibilities might be characteristic of statistical law. An attempt to analyse the differences beween these three cases would be so arduous and important a task as to completely overshadow our original thesis. Moreover, this attempt is not necessary, since the difference which holds our interest here is that between these three and the other, and this difference is found in the similarity of the three. Whereas in the one case the conclusion is 'certain', in each of these others it is only probable. And it is this probability in the conclusion which we take to be characteristic of statistical law.

The previous paragraph merely suggests a problem. It seems to be fair to make the same distinctions between the two types of law as between the two different cases of inference. If the analogy holds, in the form in which it has been expressed above, then it is important that the three different types of statistical law be analyzed and reduced to some foundation, along with the different types of inference. Deductive logic has dealt with the one type. Some science whose foundations have yet to be laid in a system of postulates must deal with these other three

And it seems that this science must start out by recognizing differences in the nature of the premisses as well as in that of the inference.

These analogies, however vague, serve to point out a definite difference between mechanical and statistical law: the difference in the sense in which each may be said to predict. Mechanical law we found to predict a definite state of affairs, analogous to a 'certain' conclusion. Statistical law, on the other hand, whether the statistics enter by way of the fact or the law, must always predict a probability. And though this probability be perfectly definite, the prediction is essentially different from that of mechanical law. Further consideration of this difference leads us to our third point, that of the sense in which statistical law is true.

Since Aristotle, there has been a distinction made between propositions whose truth and falsity are contradictory, and propositions which are true 'for the most part'. Propositions which involve probabilities seem to fall into the latter category. A prediction arising out of a mechanical law takes the form of a proposition—"Such and such is the case." Experiment decides, within the limits of error, whether this proposition is true or false. Statistical law, on the other hand, predicts that "Such and such is probably the case." Experiment can determine, as before, whether or not "such and such is the case," but can say nothing about the probability. In the case of the predictions of mechanical law, confirmation by experiment was not final, but was evidence of a sort that the law was valid, while invalidation of the law resulted from a single negative result. If experiment should show that what was predicted as probable by a statistical law actually turned out to be true, this would again be evidence, though not final, of the law's validity. If experiment should show what was

predicted as probable to be not true, there would not be sufficient cause to reject the law. We would recognize here an example of a probability which 'did not realize itself', and this seems to be the inalienable right of all probabilities.

So much for statistical law and the features that distinguish it from the other sort. In regard to the question as to whether description in terms of statistical law may be always possible, it is the first of these features which is important. The second is mentioned merely because of the interest attaching to the difficulties associated with it, and the third because of the bearing which it might have on the desirability of this sort of description, especially in connection with the edict of Poincaré, that ".... above all, the scientist must foresee." The attitude of one who is seeking a description in terms of statistical law towards the entities he is seeking to describe seems to be the crucial point. Is this attitude possible, or can anything make it impossible? There seems to be no difficulty in finding an answer. Common sense and common practice seem to show that, wherever there are individuals which fall into some general classification, it is possible to find some law of average or statistical behaviour for these individuals, in terms of a probability associated with all or any of them. All that is necessary is the ability to classify, and one sees no ground for denying this. Even though there be but a single individual concerned, as this coin which I am about to toss, it is still possible to arrive at a law of its probable behaviour, by considering this individual a number of times, under conditions which can be classified as similar. As soon as it is possible to postulate a group of 'similar' objects, or an object under 'similar' conditions, there is room for a statistical law, either for the behaviour of the group, or of the individuals in the group,

or of the individual which is repeatedly subject to 'similar' circumstances.

An interesting example of such procedure might find a place here. Suppose that one were aware of the heavenly bodies only through shadows projected upon a two-dimensional screen. One would become interested in their behaviour and attempt in some way to describe it. Paths might be traced out and velocities noted, and some sort of law and order introduced into what at first might appear to be confusion. But every now and then there would be an occurrence which would fit in with no orderly description at all. For instance, it might be that sometimes when two of these bodies arrived at the same point at the same time, there would be an explosion, while at other times each would continue in its path as though the other were not there. Finding no way to separate 'some' times, from 'other' times, the describer might go through his data and determine what percentage of all meetings resulted in explosions. He might then declare that for each body there is a certain probability of its collision with another being harmless. Or he might go further, and considering certain ones of these bodies which had some feature in common, find the same sort of probability for these. The only cases in which he could not do this would be where all, one only, or no collisions had a disastrous effect.

Lest this seem to tell the whole story, and leave one to suppose that we have here an example where mechanical description is out of the question, another way of dealing with the situation must be pointed out. Our describer has only to invent and introduce into his system a third dimension, in order to be able to find a description which predicts all occurrences with certainty.

Our efforts have been centered upon making explicit the essential features of two attitudes which may be taken

toward the description of nature. Consideration of these features seems to show that either attitude is a possible one, and that nothing that experience can produce will ever make it necessary to reject one or the other. Our attitude toward the question of description may then be summed up in a few words.

These words need not be our own. We may borrow a paragraph from Pierre Duhem which aptly expressed our meaning, although its statements are based on argument somewhat different from our own. "Thus, for anyone who confines himself to the procedures of the experimental method, it is impossible to assert the truth of the proposition: All physical phenomena are explained mechanically. It is equally impossible to declare it false. This proposition transcends the method of physics."[20] And of course, the same is to be said for another proposition which would make all physical description statistical. The question, then, as to which sort of description is to be employed, is one that transcends experience. Does this mean that the question is meaningless? On another page, a distinction was made between questions which, to have meaning, must find some answer in experience, and questions for which this is not so. Our question can find no answer in experience, but it is not the sort of question which demands such an answer. Instead of becoming a meaningless question of fact, it becomes a question, not of fact at all, but of motive, of purpose. For its answer will appear in hypothetical form,—"If I am seeking certain ends, then I will choose this manner of describing nature."

The situation is analogous to many another in the history of science. Shall I, in Ptolemaic fashion, take the earth to be fixed and immovable at the center of the planetary system, and describe the motion of the planets

20P. Duhem, *l'Évolution de la Mécanique*, p. 184.

and the sun with respect to it, or shall I, with Copernicus, set up my coördinate system upon the sun? This is hardly a question of fact, to be answered by an appeal to experience. Rather, its answering depends upon which influences me more, a desire to stand well with my medieval Church, or a desire to find the simplest and easiest way of describing these natural motions. Shall I account for the phenomena of gravitation by some hypothesis involving the impacts of ultramundane particles, or in terms of action as a distance? Shall I insist on the axioms of Euclid as true, or shall I adhere to those of Riemann or Lobatschewsky? If regarded as questions of fact, these and many others become meaningless. But if an answer is to be sought in terms of some motive, they become questions whose meaning is important.

Having reached this point, where the answer to one question seems to depend on the answer to another, we might go on to point out certain ends that seem eminently desirable and which can only be attained if we deal with nature as a mechanical system. But we shall rest satisfied with having defined to a certain extent the issue between the two sorts of description, and having shown this issue to be one not of fact, dependent on the state of our knowledge, but of motive, dependent on the state of our soul.

We shall proceed, then, to an account of recent developments in physics which seem to make the issue one of fact, and criticize, from the point of view which we have achieved, certain interpretations of these developments.

IV

To go through the steps which have led up to the present situation in atomic theory would be a task far beyond the scope of this paper. A whole great block of history will be avoided by summing up the state of affairs

in 1925, when the atom was regarded as a miniature planetary system, with a comparatively heavy nucleus about which electrons revolved according to the Newtonian inverse square law. The only modification of this simple conception necessary to attain the idea of the then prevailing "Bohr model" of the atom is the introduction of the phenomenon associated with the word "quantum."

We can pass by with simple mention the birth of the quantum idea in Planck's considerations of the laws of heat radiation, when an element of discontinuity was introduced into the description of atomic processes to account for the empirical laws of radiation. The idea can be expressed in the following words as well as in any others. "Planck discovered that in the statistical behaviour of particles in simple harmonic oscillation about a point of equilibrium, only such states of vibration must be taken into account, the energy of which is an integral multiple of a 'quantum' $h\omega$, where ω is the frequency of the particle and h is a universal constant, Planck's quantum of action."[21]

To illustrate the necessity of this radical "quantum" modification of the simple notion of a "planetary system," some explanation is needed. This may be most easily set forth by considering the nature of the evidence on which theories of the structure of atoms is based. One of the principal sources of evidence is to be found in the line spectra of the various elements. If the motion of the electrons about the nucleus in an atom is describable by a law of the Newtonian sort, then this motion should be continuous in itself and in its changes. In other words, it should be possible for the electrons to revolve in any orbits, depending upon the energy content of the atom. And this means that the spectrum produced by a quantity of

21N. Bohr, "Atomic Theory and Mechanics," *Nature*, Dec. 5, 1925. Supplement.

a particular element, in which the atoms may be in any state, would give an almost continuous spectrum, the wave-length of the light emitted depending upon the period of electronic revolution, hence upon the energy associated with the atom. But what is actually found is that a given element produces a series of lines as its spectrum, and these lines and their arrangement give evidence from which calculations can be made as to the energy state of the atoms.

It should perhaps be remarked here that it is always an aggregate of atoms with which one has to do; that nothing may be said in this respect of a single atom; and that each line is to be taken as produced by a number of atoms in the same state. The percentage of atoms in a particular state, or the probability of any one atom being in that state, is determined by the relative intensity of the corresponding line. Under these conditions, given a finite number of lines, it is to be concluded that there is only a discrete sequence of energy states possible for the atoms of the particular element. For calculation shows that if the emission of light is taken to be evidence of changes of energy through the passage of an electron from one orbit to another, then electrons can only occupy orbits the passage between which involves a change of energy which is equal to the product of Planck's quantum number and the frequency of the emitted light.

We have been thinking and expressing ourselves in terms of the picture offered by Bohr's hypothetical model of the atom. But this model, having served a very useful purpose, has had to fall into the discard, owing to the difficulties encountered when more than one electron is to be accounted for. The idea remains without the model, however, and we may speak of certain energy states of the

atom without thinking in terms of orbits and planetary motion.

Bohr has summed up the situation in the form of two postulates to overcome the difficulties presented by this divergence from our experience with other phenomena. He states them as follows,—

(1). "An atomic system possesses a certain manifold of states (the stationary states), to which correspond in general a discrete sequence of energy values and which have a peculiar stability. Every change in energy is due to a transition from one stationary state to another.

(2). "The possibility of emission and absorption of radiation by the atom is conditioned by the possibility of energy changes of the atom, in such a way that the frequency of radiation is connected with the energy difference between the initial and final state by the formal relation $h\nu = E_1 - E_2$."[22]

(ν is the radiation frequency. h is Planck's quantum.)

And he goes on to say, "These postulates, while not explainable on classical ideas, seem to offer a basis for a general account of the observed physical and chemical properties of the elements."

The account to which he refers takes the form of the "new quantum mechanics," a consideration of which will bring us back to our problem.

The attempt at a rational quantum mechanics, which is most closely associated with the name of Heisenberg[23], is an effort to avoid the difficulty resulting from trying to represent atomic motion in the usual pictorial fashion. The prime element of its rationality is seen in its author's description. "The quantum mechanics originated from the attempt to break with the former usual kinematic concepts

[22]N. Bohr, *loc. cit.*

[23]W. Heisenberg, and Jordan, *Zeits. f. Physik,* Vol. 34, p. 858, and Vol. 35, p. 557.

and put in their place relations between concrete experimentally given numbers."[24] The theory begins with what we have called the quantum postulates, and operates with manifolds of quantities, which symbolize the possibilities of transitions between stationary states, in conformity with the "correspondence principle," "according to which the possibility of every transition process connected with the emission of radiation is conditioned by the presence of a corresponding harmonic component in the motion of the atom."[25]

Given this motive, these primary conditions, it is not important that for Heisenberg this quantum mechanics finds expression in the form of matrices whose indices refer to definite stationary states or energy levels, and whose elements are Fourier's series. For others it is in the form of wave functions or of q-numbers, or operators or transformations, and certain mathematical relations of equivalence tie them together. What is important is the conclusion, which has to take the form of a fundamental principle, and which is forced by the primary conditions.

"There is one fundamental physical lemma which epitomizes the spirit of the whole statistical outlook, just as the invariance of the velocity of light does the spirit of relativity or the second law that "heat doesn't flow uphill" does the spirit of thermodynamics. This lemma is 'Heisenberg's indeterminism principle'."[26]

The principle is simple enough in statement. "It is impossible to specify or measure accurately and simultaneously both a coordinate 'q' and its conjugate momentum 'p'."[26] Or more exactly, we may say that the product of the er-

[24]W. Heisenberg, *"Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik, Zeits. f. Physik,* 43, 1927, p. 172.

[25]N. Bohr, *loc. cit.*

[26]J. H. Van Vleck, *Journal of the Franklin Institute,* Vol. 207, no. 4, April, 1929, p. 476.

rors 'Δp', 'Δq', in determining these quantities is of the order of Planck's quantum h; that is $\Delta p \Delta q \sim h$. If, to take a special case, the 'q' is a Cartesian coördinate and 'p'='mv', then the principle states that it is impossible to specify simultaneously the position and the velocity of an electron.

The situation is a critical one. In a previous discussion we have considered experimental error as something which might always be reduced, and here we find a reciprocal relation such that, as the error in the determination of one quantity is reduced, that of another necessarily increases. And in the limiting case, if one quantity were supposed to be known exactly, nothing could be said about the other.

Many things seem open to question. Not only the possibility of unlimited reduction of error and of definition of the 'real' as a limiting conception. There is besides, the whole problem of mechanical description of the physical world, which depends on the simultaneous determination of mass, position, and velocity (or some such dimensions).

Nothing has been said of the grounds for formulating this principle. They are found in the experiment (any possible experiment) which attempts the determination in question. All this, which is becoming an old story, finds discussion in Heisenberg's paper of 1927, and we may follow that writer for a while. It is easy to make clear his motive by quoting extensively from the introduction to the paper.

"But that a revision of the kinematic and mechanical concepts is necessary appears to follow immediately from the fundamental equations of the quantum mechanics. If a particular mass (m) is given, it has a simple, understandable meaning to speak of the position and of the velocity of the center of gravity of this mass (m). But in the

quantum mechanics a relation $pq - qp = \frac{h}{2\pi i}$ [27] must exist between mass, position, and velocity. We have, therefore, good ground to conceive a suspicion against the uncritical application of these words "position" and "velocity." If one admits that discontinuities are typical of events in very small spaces and times, then even a denial of the concepts position and velocity is immediately possible; for example, if one thinks of the one-dimensional motion of a mass-point, then in a continuum-theory one will be able to show a path-curve x(t) for the path of the particule (more accurately: its center of gravity), the tangent giving the velocity. On the other hand, in a discontinuum-theory, a series of points at finite distances will take the place of this curve. In this case it is apparently meaningless to speak of the velocity at a particular point, because first the velocity can be defined through two points, and then conversely, to each point belong two different velocities.

"Hence the question arises, whether it is not possible, through a strict analysis of those kinematic and mechanical concepts, to clear up the till now existing contradictions in the graphic[28] interpretation of the quantum mechanics, and to come to a graphic comprehension of the quantum mechanical relations."

So a section is devoted to the concepts path, position, velocity, energy; and experiments for the determination of these quantities are discussed.

At this point it is essential to quote again, just a sin-

[27] It must be remembered that the p and q stand for matrices, and that multiplication of matrices is non-commutative.

[28] In the original, *anschaulichen*. Its meaning is made explicit in the opening sentence of Heisenberg's paper,—*"Eine physikalische Theorie glauben wir dann anschaulich zu verstehen, wenn wir uns in allen einfachen Fällen die experimentellen Konsequenzen dieser Theorie qualitativ denken können, und wenn wir gleichzeitig erkannt haben, dass die Anwendung der Theorie niemals innere Widersprüche enthält."*

gle sentence. "If one wishes to be clear as to what is understood by the words 'position of an object'; of an electron, for example, (relative to a given reference system), one must specify a particular experiment with the help of which one intends to measure the 'position of the electron'; otherwise these words have no sense." It seems necessary to make this explicit, since the point of view is one which is receiving much consideration in the world of the physicist, as well as because it plays such a basic part in this paper of Heisenberg. In this country it has been supported by P. W. Bridgman in his work, *The Logic of Modern Physics,* where it is called the 'operational point of view'.

We come, then, to the experiment which is to determine the "position of the electron." "But in principle one may build a gamma-ray microscope, and with this be able to accomplish the position-determination as accurately as one will. There is in this determination an accessory circumstance which is essential, the Compton effect. Every observation of a light ray coming from an electron presupposes a light-electrical effect (in the eye, on the photographic plate, in the photo-cell) and can therefore also be interpreted thus, that a light-quanta hit the electron, was reflected or turned aside from it, and then, diverted by the lens of the microscope, produced the photo-effect. In the instant of the position-determination, therefore in the instant in which the light quantum is turned aside from the electron, the electron changes its unsteady momentum. This change is greater, the shorter the wavelength of the light used; that is, the more accurate the phase-determination. In the moment in which the position of the electron is known, its momentum can only be known up to the quantities which correspond to this unsteady change; therefore the more accurately the position is de-

termined, just so inexactly is the momentum known, and conversely. Herein we see direct (*anschauliche*) illustration of the relation $pq - qp = \frac{h}{2\pi i}$. Let q_1 be the accuracy with which the value q is known (q_1 is the mean error of q), therefore here the wave length of the light; let p_1 be the accuracy with which the value p is determinable, therefore here the unsteady change of p by the Compton effect; then according to the elementary formula of the Compton effect, p_1 and q_1 stand in the relation $p_1 q_1 \sim h$ (1)."

All this reduces pretty well to a statement that in any experiment there is an interaction beween the object observed and the observer (or his apparatus) and that this interaction prevents the simultaneous determination of two conjugate quantities.

Much the same treatment is given to the concept "path of an electron." The electron must be illuminated, for the determination of one point in its path, and the illumination throws the electron out of this path.

"In this, that in the quantum theory, only the probability function of the electron's position can be given, for a particular state, 1-S for instance, one may see with Born and Jordan a characteristic statistical feature of the quantum theory in contradiction of the classical theory. But one can, if he will, also say with Dirac that the statistics is brought in by our experiment. For apparently in the classical theory also only the probability of a particular electron-position was specifiable, so long as we did not know the phase of the atom. The difference between the classical and the quantum mechanics rests much more on this: Classically, we can always think of the phase as determined in the foregoing experiment; in reality this is impossible, because any experiment for the determination of the phase destroys or changes the atom."

From the same sort of considerations it is seen too that "velocity of an electron" cannot be defined for a given state of the atom, and that in measurements of energy we are led to a relation $E_1 t_1 \sim h$, which corresponds to relation (1).

If, now, we turn to the conclusion of Heisenberg's paper, we will see what that writer makes of these results.

"If one accepts that the interpretation of the quantum mechanics attempted here is in its essential points correct, then it may be permitted to proceed in a few words to its principal consequences. That the quantum theory is, in opposition to the classical theory, an essentially statistical one, in the sense that from the exact given data only statistical conclusions can be drawn, we have not assumed. Against such an assumption speak, for example, the well-known experiments of Geiger and Bothe. In all cases in which the relations of the classical theory stand between magnitudes which are really exactly measurable, the corresponding exact relations in the quantum theory (momentum and energy laws) are much more valid. But in the strict formulation of the causality law: 'If we know the present exactly we can calculate the future,' it is not the consequent, but the presupposition, that is false. We cannot learn to know the present in all respects. Therefore all perception is a selection from a plenitude of possibilities, and a limitation of future possibilities. Since now the statistical character of the quantum theory is so closely connected to the inaccuracy of all perception, one can be misled to the supposition that the 'real' world, in which the causality law is valid, hides under the perceived world. But such speculations appear to us, we emphasize expressly, barren and senseless. Physics must formally describe only the connections of perceptions. Much better can one characterize the true facts so: Because all experiments are subject to the laws of the quantum mechanics, and so to

equation (1), then the invalidity of the causality law is definitely fixed by the quantum theory."

V

In offering by way of conclusion some criticism of the attitude toward physical science which has grown out of the statement of Heisenberg's 'indeterminism principle,' it seems best that we confine ourslves to particular illustrations of that attitude. What we intend is not a critical examination of the theory on which Heisenberg bases his conclusions. Such an examination would have to investigate, at every point in the development of theories of the structure of matter, the motive for making some particular choice rather than any other. Why must there be discontinuities? Why is the notion of the stationary state so absolutely essential? And so on. It is not these historical choices, but those later ones which have aroused contemporary interest, and whose bid for newness makes them seem so strange, that are the subject of our comment.

There is just one point in connection with the technical development of the theory which we shall mention here, and consideration of it may be brief. The experiment whose failure to determine unambiguously the "position of the electron" makes the usual concept "position" a meaningless one, is entirely a "thought-experiment." Its theory is based on the Compton effect; it is a theoretical interpretation of certain experimental results. Because we are not prepared to support the contention, we do not suggest here that this procedure, which allows an interpretation of an experimental result to impose conditions upon experiment in general, is a case of begging the question. But it does seem necessary to point out that, before Heisenberg's argument can be accepted, the conditions which

are assumed in this particular "thought-experiment" must be proved to be necessary and universal conditions of all observation and experiment. Having made this comment, we may turn to matters of more general import.

At an early stage in the development of our subject, we found place for an argument which goes back to Kant. The term observation, we said, can have no meaning apart from a "something observed." We have found Heisenberg saying that speculations such as "that the 'real' world, in which the causality law is valid, hides under the perceived statistical world" appear to him as "barren and senseless." We feel that he has made a mistake in using the word 'hides'. That the existence of a real world is necessary for there to be perception of any world at all is a diffrent way of looking at the matter, and one not to be so easily escaped. To say that all we can know is appearance is one thing; to say that all there can be is appearance is another; an assertion of the latter is a contradiction in terms.

Nor is it sufficient for Heisenberg to say that "physics must formally describe only the connection of perceptions." For one of the tasks of physics has always been to reduce perceptions to certain common elements,—called dimensions,—and one of its guiding motives has been the reduction of the number of these dimensions to as few as possible. This motive might be found to operate in the attempts of a Democritus or a Descartes to explain all phenomena in terms of the motion of particles. Or its presence might be discovered behind the investigations which led to the discovery of a 'mechanical equivalent of heat', or the development of a 'kinetic theory of gases.' The simplicity which is set up by so many writers as the guidepost directing scientific choice is not itself a simple thing. As has been suggested before, it may be a simplicity re-

siding in the form of the law, or one which results from a single law serving the purposes of two. Or it may be the simplicity of a description which requires fewer independent properties to be linked together in a law. All these have proved themselves worthy motives, and the history of science is, in a sense, the record of a struggle between rival simplicities. It is apparent from Heisenberg's paper that, for the sake of progress, he is giving up the ideal of simplicity in dimensions, in order to pursue some other.

There have been plenty of periods in history which, finding their progress in one direction arrested, have turned to a qualitative account of things for some suggestion as to how to proceed. There are those who claim that all advancement in science is due to the intuition of some ready genius, to some 'flair' or some 'feeling' for things in their qualitative aspect. One would take this to be particularly true of a branch of science which, because still in a comparatively juvenile state of development, is seeking any sort of a theoretical basis on which to proceed and from which to depart.

Thirty years or so ago we find Duhem to remark that the new physics rids itself of certain exigencies which cramped the old. "Among these exigencies the first and most essential is that which pretends to reduce all the properties of bodies to magnitudes, forms, and local motions. The new physics rejects this pretention; it admits in its reasoning the consideration of qualities; it restores to the notion of movement all the generality which was attributed to it by Aristotle. And here is the secret of its marvelous flexibility...."[29] And again,—"This deep chasm which separates directly observable qualities from the geometric and mechanical magnitudes to which one pretends to reduce them brands mechanistic theories with

[29] P. Duhem, *L'Évolution de la Mécanique*, Paris, 1903, p. 344.

a stamp so essential and evident that all the adversaries of mechanism have found here its weak spot, the defect in its armor at which their attacks should be aimed. The constant reproach which they direct at the doctrine which they wish to destroy is that this doctrine, in order to fill up the gaping chasm, must arbitrarily contrive the most complicated arrangements, and accumulate *hidden masses* and *hidden motions*. When Newton issued his famous *hypotheses non fingo,* it is precisely this task which he was refusing to undertake."[30]

Whether or not this is what Newton meant by his famous assertion would be a pleasant point to argue. One might quote a part of the sentence immediately preceding it in the *Principia,* to the effect that "Gravitation towards the sun is made up of the gravitations toward the several particles of which the body of the sun is composed:. . . .," and accuse Newton of having himself to do with unobservable quantities.

But this being as it may, it is still true that after Newton, from Boscovich and Le Sage down to Hertz, the desire for keeping the number of dimensions to as few as possible was a guiding motive in the exercising of scientific choices. And it is hard to imagine that this desire be ever wholly dead. Circumstances may make it advisable now and then to build up a theory which involves only relations "between concrete experimentally given numbers," especially when the theory has to do with the 'very small'. But must one rest content with this? The past has found itself restless and dissatisfied in such a state. And why should not the future? Especially when it feels an urge to bring this theory into harmony with views of nature which are based upon consideration of the 'very large'.

30P. Duhem, *La Valeur de la Théorie Physique,* included in *La Théorie Physique,"* 2nd. ed., Paris, 1914, p. 491.

When Heisenberg, or any other, says that positive science must explain only perceptions, or deal with only directly observable quantities, we must agree, if we can understand him to mean, as we think Newton meant, that, for science, a question asking *why?* is to be answered by telling *how*. But if he means that the concepts to be admitted into a science or the properties to be granted objects, or the dimensions of nature, are limited to those which are capable of measurement in every experiment, or are extended to include those which may be measured in any experiment, then our faith in the guide-post of history, as well as a world-old desire for a particular sort of simplicity, force us to dissent.

There remains but a single point to discuss: Heisenberg's criticism of what he calls the 'causality law.' "If we know the present exactly, we can calculate the future." For him this law breaks down because "We cannot learn to know the present in all respects. Therefore all perception is a selection from a plenitude of possibilities,...." These statements have both been true, and moreover, quite commonly recognized, since the time of Galileo's experiments. And the facts which they state can have no more power now than at any other time to overthrow the principle of causality or determinism in natural law. One would prefer to state the case thus,—Because all perception is a selection from a plenitude of possibilities, there is always the opportunity to select such as will conform to the determinism which characterizes mechanical description.

Heisenberg's stand on this question seems to be that of others who subscribe to the 'operational point of view'. In illustration, consider the assertion of P. Jordan, that "Physicists no longer doubt that this question of the existence of a complete causal determinism can only be settled

by experience, and that causality is not an *a priori* necessity of thought."[31] The same author also insists that "the definition of determinism means specification of conditions by which its existence may be experimentally established."[31] To look upon causal determinism as something which might or might not be experimentally established is to regard any fact or law of nature as something to be found. A recognition of the fact that no *one* thing is found in nature, but only a number of things from which one must be chosen, carries with it a realization that whether or not causal determinism is to be considered characteristic of nature is a matter of choice. Under these conditions, the sort of definition of determinism suggested above is out of the question. But this need not mean that the concept is a meaningless one. Rather, it may very well find a meaning elsewhere than in experience, as an ideal.

We are not insisting upon causality as an *a priori* necessity of thought. But we do insist that the causality law, or more accurately, the determinism found in "mechanical description," is something to be imposed upon nature, if to be found there at all. It is because of a refusal to impose this condition, rather than "because all experiments are subject to the laws of the quantum mechanics," that "the invalidity of the causality law is definitely fixed by the quantum theory."

[31] P. Jordan, "Philosophical Foundations of the Quantum Theory," *Nature*, Vol. 119, No. 2998, April 16, 1927, p. 566.

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